

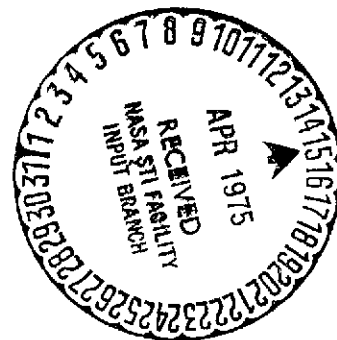
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PROCEEDINGS
OUTER PLANET PROBE TECHNOLOGY WORKSHOP
SUMMARY VOLUME

May 21-23, 1974



August, 1974



NATIONAL AERONAUTICS AND SPACE ADMINISTRATION
Ames Research Center
Moffett Field, California

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INTRODUCTION

This document has been prepared as a summary presentation and overview of the proceedings of the Outer Planet Probe Technology Workshop held at the NASA Ames Research Center, May 21-23, 1974. The Workshop was sponsored by Mr. D. Herman of the Advanced Programs and Technology Office, NASA Headquarters; and Mr. B. Padrick of the Advanced Space Projects Office, NASA Ames Research Center. The General Chairman was Mr. A. Seiff of NASA and Mr. N. Vojvodich of NASA Ames was the Technical Chairman.

The purposes of the Workshop were:

- o Review and summarize the state-of-the-art concerning mission definitions, probe requirements, systems, subsystems, and mission-peculiar hardware.
- o Explore mission and equipment trade-offs associated with a Saturn/Uranus baseline configuration and the influence of Titan and Jupiter options on both mission performance and cost.
- o Identify critically required future R&D activities.

To accomplish these purposes the Workshop was organized into ten sessions, or panels, covering the broad spectrum of science and engineering subjects concerned with the planning and implementation of in-situ measurements at the outer planets using atmospheric entry probes. Presentations of subject material were made by the participants as indicated in the program (see Section A herein). Following the session presentations, each panel convened a "splinter" meeting during which the topics, problems, etc. were discussed in more detail. The eleventh session was a summary and roundtable discussion on the concluding afternoon of the Workshop during which each panel chairman reviewed the key points covered during their respective sessions and splinter meetings.

This summary document contains:

- Section A: Program, Titles and Authors of Presentations
- Section B: The Session I Keynote Address
- Section C: The Session XI Summary Roundtable Discussion
- Section D: List of Workshop Attendees

The entire proceedings of the Workshop are currently in draft form undergoing review and editing and will be published in the very near future. This document and the proceedings have been prepared by DYNATREND INCORPORATED; Burlington, Mass. under contract to the NASA Ames Research Center.

SECTION A

PROGRAM
TITLES AND AUTHORS OF PRESENTATIONS

PROGRAM

Speaker

Subject

SESSION I - KEYNOTE ADDRESS

A. Seiff - NASA Ames	Opening remarks and introduction of Mr. Syvertson
S. Syvertson - NASA Ames	Welcome to Ames
N. Vojvodich - NASA Ames	Opening remarks and outline of workshop schedule
D. Herman - NASA Headquarters	Keynote address

SESSION II - SCIENCE RATIONALE AND OBJECTIVES

I. Rasool, Chairman NASA Headquarters	Introductory remarks
T. Owen - State University of New York	New IR Observations of Titan and Potential of In-situ Atmospheric Analysis of the Outer Planets
D. Hunten - Kitt Peak National Observatory	Diagnostic Measurements of the Upper Atmosphere
J. Lewis - Massachusetts Institute of Technology	Compositional Measurements by Outer Planet Entry Probes
J. Wolfe - NASA Ames	Pioneer 10 Jupiter Atmospheric Definition Results - A Summary
A. Kliore - Jet Propulsion Laboratory	Presentation of the Results of the Pioneer 10 Occultation Experiment
J. Moore - Jet Propulsion Laboratory	Uranus Science Planning
K. Ledbetter - Martin-Marietta Corporation	Impact of Science Objectives and Requirements on Probe Mission and System Design
H. Meyers - McDonnell-Douglas Corporation	Science Data Gathering

SpeakerSubject

SESSION III - MISSION AND SPACECRAFT DESIGN CONSTRAINTS

B. Swenson, Chairman NASA Ames	Introductory Remarks - Outer Planet Mission Analysis Overview
L. Friedman - Jet Propulsion Laboratory	Outer Planet Probe Navigation
W. Dixon - TRW Systems Group	The Pioneer Spacecraft as a Probe Carrier
J. Hyde - Jet Propulsion Laboratory	The Mariner Spacecraft as a Probe Carrier
T. Hendricks - Martin-Marietta Corporation	The Impact of Mission Requirements on System Design
C. Hinrichs - McDonnell-Doug- las Corporation	Probe Communications Constraints Imposed by Mission Parameters for a Typical Jupiter Mission

SESSION IV - PROBE DESIGN AND SYSTEM INTEGRATION

T. Canning, Chairman NASA Ames	Introductory Remarks
T. Ellis - DYNATREND	Ten Bar Probe Technical Summary
J. Goodlette - Martin- Marietta Corporation	Viking Lander Design and System Integration
L. Nolte - Hughes Aircraft Corporation	Pioneer Venus Probe Design
E. Casani - Jet Propulsion Laboratory	Probe Interface Design Consider- ations
W. Cowan - McDonnell-Douglas Corporation	Probe Design
P. Carroll - Martin-Marietta Corporation	Probe Design and System Integra- tion

SpeakerSubject

SESSION V - ENTRY AERODYNAMICS AND HEATING

W. Olstad, Chairman NASA Langley	Introductory Remarks
D. Kirk- NASA Ames	Effect of Initial Conditions on Deduced Atmosphere for Uranus and Jupiter Entries
L. Leibowitz - Jet Propulsion Laboratory	Radiative Relaxation Rates and In- tensities During Outer Planet Entries
T. Kuo - Jet Propulsion Labo- ratory	Nonequilibrium Shock-Layer Compu- tation for Saturn Probes
R. Polutchko - Martin-Marietta Corporation	Viking Entry Aerodynamics and Heating
G. Walberg - NASA Langley	Calculation of Downstream Radia- tive Flow Fields with Massive Ablation
W. Nicolet - Aerotherm	Aerothermal Environment and Material Response, A Review

SESSION VI - HEAT PROTECTION

P. Nachtsheim, Chairman NASA Ames	Introductory Remarks
S. Mezines - McDonnell-Doug- las Corporation	Carbon Phenolic Heat Shields for Jupiter/Saturn/Uranus Entry Probes
J. Lundell - NASA Ames	A Survey of the Ablation of Graphic Materials in Severe Heating En- vironments
W. Congdon - Martin-Marietta Corporation	Major Uncertainties Influencing Entry Probe Heat Shield Design
J. Howe - NASA Ames	Performance of Reflecting Silica Heat Shields During Entry into Saturn and Uranus
J. Blome - McDonnell-Douglas Corporation	High Purity Silica Reflective Heat Shield Development
H. Stine - NASA Ames	Ames Facilities for Simulating Planetary Probe Heating Environments

SpeakerSubject

SESSION VII - COMMUNICATIONS AND DATA HANDLING

T. Grant, Chairman NASA Ames	Introductory remarks
R. Compton - Martin-Marietta Corporation	Microwave Propagation in the Atmospheres of the Outer Planets
P. Parsons - Jet Propulsion Laboratory	A Data Link Relay Design
C. Hinrichs - McDonnell-Doug- las Corporation	Digital Receiver Simulation
J. Modestino - Rensselaer Polytechnic Institute	Convolutional Code Performance in Planetary Entry Channels
T. Croft - Stanford University	Radio-Frequency Science Consider- ations

SESSION VIII - SCIENCE INSTRUMENTS

J. Sperans, Chairman NASA Ames	Introductory Remarks
A. Nier - University of Minnesota	Determination of the Composition of Rarefied Neutral Atmospheres by Mass Spectrometers Carried on High-Speed Spacecraft
N. Spencer - NASA Goddard	A Mass Spectrometer Concept for Identifying Planetary Atmosphere Composition
J. Hoffman - University of Texas	Mass Spectrometric Measurements of Atmospheric Composition
S. Sommer - NASA Ames	Comparative Atmosphere Struc- ture Experiment
W. Kessler - McDonnell-Doug- las Corporation	Impact of the Retained Heat Shield Concept on Science Instruments
B. Ragent - NASA Ames	Cloud-Detecting Nephelometer for Pioneer Venus Probes
V. Oyama - NASA Ames	An Application of Gas Chromato- graphic Analysis to the Atmospheres of Saturn and Uranus

SpeakerSubject

SESSION IX - SPECIAL SUBSYSTEM DESIGN PROBLEMS

R. Toms, Chairman Jet Propulsion Laboratory	Introductory remarks
A. Hoffman - Jet Propulsion Laboratory	An Overview of Planetary Quarantine Considerations for Outer Planet Probes
R. DeFrees - McDonnell-Douglas Corporation	Planetary Quarantine Impacts on Probe Design
R. Howell - Martin-Marietta Corporation	Viking Planetary Quarantine Procedures and Implementation
L. Thayne - Martin-Marietta Corporation	Radiation Effects
E. Divita - Jet Propulsion Laboratory	The Jupiter Electron Radiation Environment and its Effects on State-of-the-Art Materials, Piece-Parts and Components
R. McMordie - Martin-Marietta Corporation	Thermal Control for Planetary Probes

SESSION X - MISSION COST ESTIMATION

N. Vojvodich, Chairman NASA Ames	Introductory Remarks
J. Niehoff - Science Applications, Inc.	Comparison of Probe Subsystem Costs with Spacecraft Subsystem Costs
B. Ruhland - Jet Propulsion Laboratory	Cost Modeling Techniques for Design Maturity
F. Bradley - McDonnell-Douglas Corporation	Systematic Approach to Design to Cost

SESSION XI - SUMMARY ROUNDTABLE DISCUSSIONS

A. Seiff, Chairman NASA Ames	Introductory Remarks
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Participants: J. Foster - NASA Ames
P. Tarver - NASA Headquarters
All Session Chairmen

SECTION B

SESSION I - KEYNOTE ADDRESS

SESSION I - KEYNOTE ADDRESS, TUESDAY, MAY 21, 1974:

Introduction by Mr. A. Seiff of NASA Ames Research Center, the General Chairman of this Workshop.

MR. SEIFF: Dr. Hans Mark is not going to be with us this morning. He was required to be in a meeting at Boulder, Colorado but is very ably represented by Si Syvertson.

I would just like to say a word or two to introduce Si even though I think most of you know him. But for those of you who don't, he speaks with some authority in the business of entry technology for the reason that maybe ten or fifteen years ago he was one of the group of people who were working on the early lifting reentry bodies at Ames which were called M-1, M-2 and so on. He has also been in the advance mission business because for a period of time he was the Chief of the Mission Analysis Division, stationed at Ames and reporting to NASA Headquarters. So Si, would you please say a few words to the group here?

MR. SYVERTSON: I'm glad Al can remember when I used to do useful things for a living. It's kind of surprising, and gratifying, to see the size of the turnout to this Workshop. We don't often get this many people in this kind of an area anymore. We are very happy to see everybody here.

As Al indicated, Ames has been interested in entry technology for a long time, going back, I guess, more than twenty years when Harvey Allen first got us started in the business. In more recent years we have been more interested in applying what we've learned rather than in the basic research areas. As everybody here is aware, we are embarking on the Pioneer-Venus program that will send multiple probes into Venus in a few years.

Later today, or tomorrow, you will hear some of the preliminary results from Pioneer 10 with regard to defining the

atmosphere on Jupiter. My understanding is that the preliminary results indicate that the entry problem there is not quite so severe as we once thought it was. I understand you are going to be looking at probes for other missions to the outer planets.

I've looked over the schedule and it looks like a very interesting meeting. I hope you enjoy it and I hope you find it informative.

On behalf of Dr. Hans Mark and the rest of the Center, I want to welcome you here to Ames. Thank you.

MR. SEIFF: This is probably the first meeting of a technical nature that I've ever attended that has a Keynote Address. It is going to be made by a man who is parked illegally, I was just informed a few minutes ago. This address is to be given by Dan Herman who has been with the Headquarters NASA Office of Space Sciences for many years. During that whole period, I have felt that he has been a real sparkplug in keeping the Agency moving towards the definition of its future programs. He has been president of practically all, if not all, of the Pioneer-Venus Science Steering Group meetings and playing an active role in the implementation of that project as well. So, Dan is going to talk to us a little bit about what he thinks the prospects are for Outer Planet Probe Missions.

ORIGINAL PAGE IS
OF POOR QUALITY

KEYNOTE ADDRESS
MR. DANIEL HERMAN - NASA HEADQUARTERS

MR. HERMAN: I am not really going to give a keynote address in the formal sense of the word. Rather, what I thought I would do is to tell you what the current status within NASA is for an outer planets probe program.

I will begin with this first picture (Figure 1-1) of the so-called official NASA mission model as of last October. These are the missions Dr. Fletcher presented to the Congress in his testimony in October and have been carried on the books as the official NASA plan. Currently, this plan is in the process of being changed because our thinking with respect to the outer planet probe missions has changed. I will indicate the changes from this so-called official NASA mission model of last October to our current thinking.

Originally, the outer planet probe missions in our plan were those stipulated by the Outer Planet Science Advisory Group, headed by Jim Van Allen. The so-called "three to make two" concept where in three opportunities dedicated Pioneer probe missions are launched to Saturn and Uranus, with the last one to either Saturn, Uranus or Titan as a function of the success or failure of the two predecessors. This strategy of the "October plan" is shown on the second schedule (Figure 1-2).

In 1979, we would send a dedicated Uranus probe mission to fly by Jupiter and be deflected to Uranus. The arrival at Uranus would be 1984. Then, in the 1980 opportunity, we would send a probe to Saturn directly and that probe would reach Saturn in 1984. Then in 1981, we would launch a probe mission, the Saturn-Uranus swing-by opportunity, which would reach Saturn in 1985 after both earlier probes had encountered Saturn and Uranus. If both earlier

PLANETARY EXPLORATION PROGRAM (PL)

October 1973

Payload Code	Payload	CY	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	Total
<u>Approved Programs</u>																						
PL-1	Mariner Venus/Mercury		①																			1
PL-2	Pioneer Jupiter Flyby		△																			0
PL-3	Helios			①		①																2
PL-4	Viking 75				②																	2
PL-5	Mariner Jup/Sat 77					②																2
<u>Inner Planets</u>																						
PL-6	Viking Orbiter/Lander 79								1													1
PL-7	Surface Sample Return													2								2
PL-8	Satellite Sample Return																		1	1		2
PL-9	Pioneer Venus							2														2
PL-10	Inner Pl. Follow-On									1	2		1			1						5
PL-11	Venus Radar Mapper											2										2
PL-12	Venus Buoyant Station													2								2
PL-13	Mercury Orbiter																2					2
PL-14	Venus Large Lander																	2				2
<u>Outer Planets</u>																						
PL-15	Mariner Jup/Uranus Flyby								2													2
PL-16	Pioneer Jup/Uranus Flyby (Uranus Probe)								1													1
PL-17	Pioneer Saturn Probe									1												1
PL-18	Pioneer Sat/Uranus Flyby (U Probe)										1											1
PL-19	Mariner Jupiter Orbiter										2											2
PL-20	Pioneer Jupiter Probe													2								2
PL-21	Mariner Saturn Orbiter														2							2
PL-22	Mariner Uranus/Nep Flyby															2						2
PL-23	Jupiter Sat. Orb/Lander																		1	1		2
<u>Comets & Asteroids</u>																						
PL-24	Dual Comet Flyby					1																1
PL-25	Encke Slow Flyby								1													1
PL-26	Encke Rendezvous									2												2
PL-27	Halley Flyby														1							1
PL-28	Asteroid Rendezvous															2						2
Total			1	1	2	2	2	2	5	2	7	0	3	4	5	5	2	0	2	2	2	49



Note:  Approved and On-going
 Launched

Figure 1-1

PROBE MISSION STRATEGY

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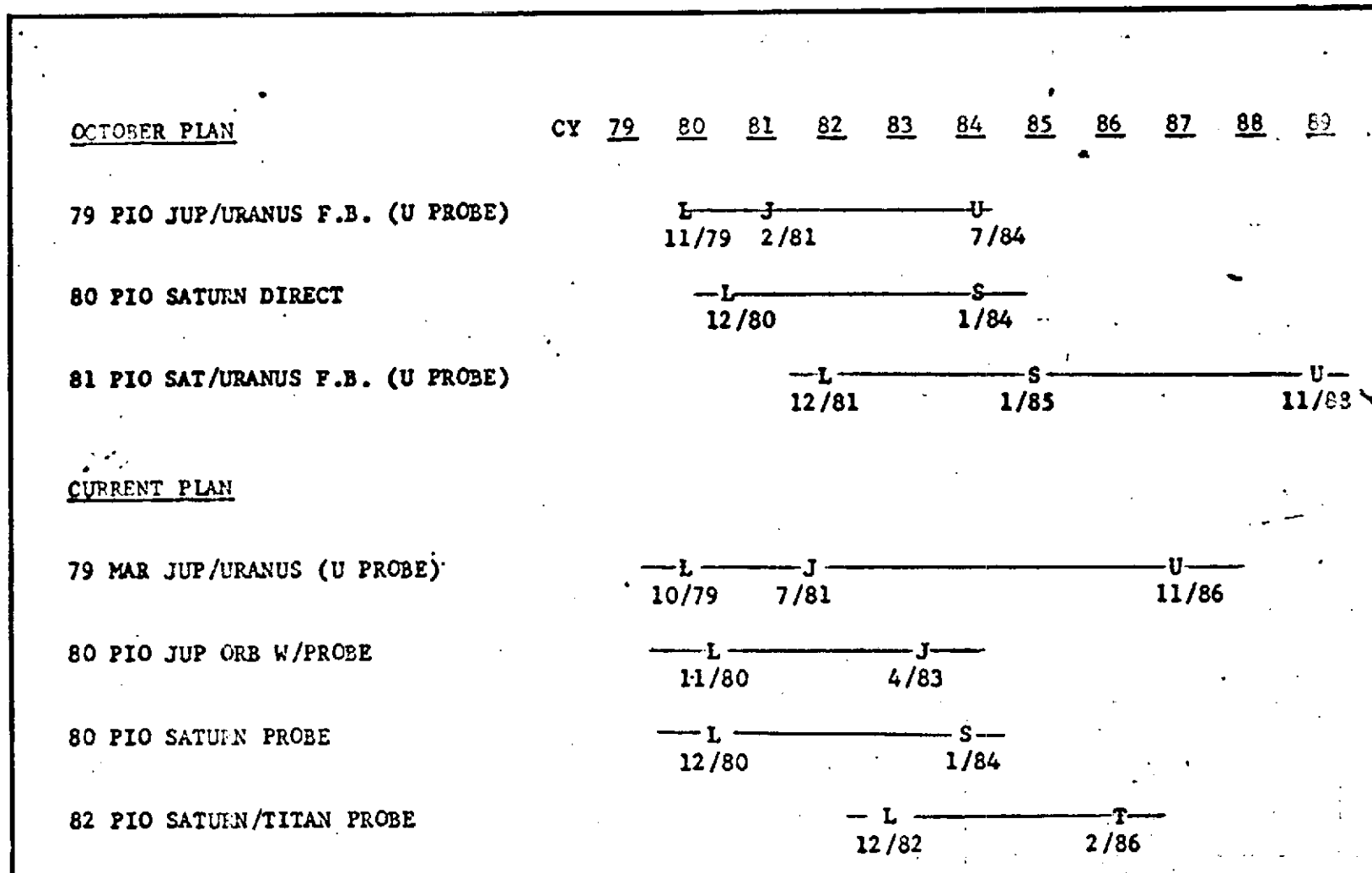


Figure 1-2

probes were successful, this probe would then go into Titan. If either the Saturn or Uranus probe was a failure, then this probe would repeat either the Saturn or the Uranus mission.

The scenario had a couple of weaknesses in it, the major one of which was exposed at the Titan workshop held here at Ames about a year or so ago. The strong advice of that workshop, which we have accepted, was we should not try to achieve commonality between a Titan probe and an outer planet high-atmosphere probe; the reasons being that the science to be performed at Titan would be different and, also, that the quarantine restraints to be imposed on a Titan probe would differ from the outer planets probe.

In this old plan (Figure 1-2) you don't see a Jupiter entry because until the Pioneer 10 encounter our entry analysis of the Jupiter probe mission, indicated that facilities would not be available until about 1980 to test an entry probe to the Jupiter entry heating conditions. Hence, we deferred a Jupiter entry probe until the mid-1980's. That thinking has changed and that is going to be a major issue of this workshop.

Let me go to this next schedule (Figure 1-3), and show you our current thinking. For the October mission model we were given a fiscal constraint by the Administrator to formulate all of the new programs we hoped to implement for the next five years. The original mission model was in consonance with that fiscal constraint. However, late last year several things happened, one of which was a forecast overrun in the Viking program.

Since our overall budget does not increase, funds for planning for new missions is from the same funding that has to accommodate overruns. We, therefore, had to alter our thinking and decide which missions we wanted to do as scheduled and which missions would have to be deferred. Since the outer planet probe missions could be done almost in any year - the opportunities to the outer planets occur in about a twelve-or fifteen-month period - these were more easily deferrable than some of our other missions.

PLANETARY EXPLORATION PROGRAMS

April 1974

PAYLOAD	CY	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91
<u>APPROVED PROGRAMS</u>																				
MARINER VENUS/MERCURY		1																		
PIONEER JUPITER FLYBY		1																		
HELIOS			1		1															
VIKING '75				2																
MARINER JUP/SAT '77					2															
<u>INNER PLANETS</u>																				
VIKING ORBITER/LANDER					1															
VIKING ORBITER/LANDER (W/ROVER)									1											
SURFACE SAMPLE RETURN													2					1	1	
SATELLITE SAMPLE RETURN																				
PIONEER VENUS					2															
INNER PLANET FOLLOW-ON									1	2		1			1					
VENUS RADAR MAPPER												2								
VENUS BUOYANT STATION													2							
MERCURY ORBITER															2					
VENUS LARGE LANDER																	2			
<u>OUTER PLANETS</u>																				
MARINER JUP/URANUS FLYBY (U PROBE)					2															
PIONEER JUPITER ORBITER (ESRO)									1											
PIONEER SATURN PROBE									2											
PIONEER SATURN/TITAN PROBE											2									
MARINER JUPITER ORBITER											2									
PIONEER JUPITER PROBE													2							
MARINER SATURN ORBITER														2						
MARINER URANUS/NEPTUNE FLYBY															2					
JUPITER SATELLITE ORB/LANDER																		1	1	
<u>COMETS AND ASTEROIDS</u>																				
ENCKE SLOW FLYBY						1														
ENCKE RENDEZVOUS									2											
HALLEY FLYBY														1						
ASTEROID RENDEZVOUS															2					

Figure 1-3

Consequently, when we formulated that mission model, the dedicated Pioneer outer planet probe missions were deferred. As I indicated before, our thinking changed about commonality between an outer planet entry probe and the Titan entry probe and, also with Pioneer 10 encounter and Arv Kliore's data about the possibility that the probe design for Saturn and Uranus would also have Jupiter capability. Since ephemeris uncertainty of Jupiter has been decreased which allows a shallow entry angle, and if the atmosphere is more toward the so-called "warm expanded" or "nominal" atmosphere, it may be possible to enter Jupiter with the same entry technology that we will use for Saturn and Uranus.

So, for several reasons, our thinking has changed. We have given up the dedicated Pioneer-Uranus entry probe. Instead, our current thinking is to incorporate a Uranus probe in a Mariner Jupiter-Uranus mission which we want to launch in 1979. As far as a Jupiter entry probe is concerned, we are discussing a cooperative program with ESRO at the present time, using Pioneer H to do an orbiter mission in the 1980 opportunity and we are going to discuss the possibility and the advisability of incorporating a Jupiter entry probe in that mission.

Our dedicated Pioneer-Saturn probes are still intact. That thinking has not changed but now you see Pioneer-Saturn-Titan probes. These would be a different kind of a probe. They would be dedicated Titan entry missions. The Pioneer-Jupiter probes is still kept on the books at the old date in case we cannot incorporate the probe into the Pioneer Jupiter orbiter mission with ESRO.

These are some concepts and some of the things that we are considering. The only way the concept of a probe on the MJU flyby is feasible is to first aim the spacecraft so that it would impact Uranus and then release the probe. The probe then need not have an attitude control system or delta-V propulsion, and after the probe is released, the spacecraft is deflected to achieve

the flyby. This mode permits use of a simple, "dumb," probe that can be developed within reasonable cost and weight constraints. However, the spacecraft deflection mode requires a new NASA policy position on the quarantine requirements for outer planet entry probes. This is being considered by the Space Science Board. This issue must be addressed since this is the only practical mode to incorporate a probe on a Mariner spacecraft to Uranus.

Figure 1-4 presents a concept of a dedicated Pioneer probe mission into Saturn. Again, the concept for probe release would be the same. The spacecraft, of course, serves as a communications relay for the probe during the entry of the probe into the atmosphere. One of the things that is being studied is the feasibility of designing one probe system which can be completely common, including science for both Saturn and Uranus.

A cooperative Jupiter mission with ESRO that I mentioned, and the possibility of a probe in that is shown here on Figure 1-5. The probe would be released before orbit capture and the spacecraft would serve as a relay for the probe during entry. Then the spacecraft would be captured and would achieve a highly elliptical orbit about the planet. The first formal meeting with ESRO on this mission is here at Ames on June 17 and 18.

Now, let me tell you one announcement that I think will be of interest to some people here. The Mariner Jupiter-Uranus Science Group that has been meeting is coming up with a strong position that an atmospheric entry probe will materially enhance the value of that mission. On the basis of a meeting last week, we at NASA decided that we would go out with an RFP to industry for a Phase B Study in fiscal year 1976 for an entry probe that can be used for Uranus, Saturn and, if possible, Jupiter. The RFP will be entitled, "Outer Planet Probes." The RFP will also state that the first mission for this outer planet probe family will be the MJU mission in 1979. Preceding the release of that RFP, Dr. Rasool is going to form a small science group to evaluate

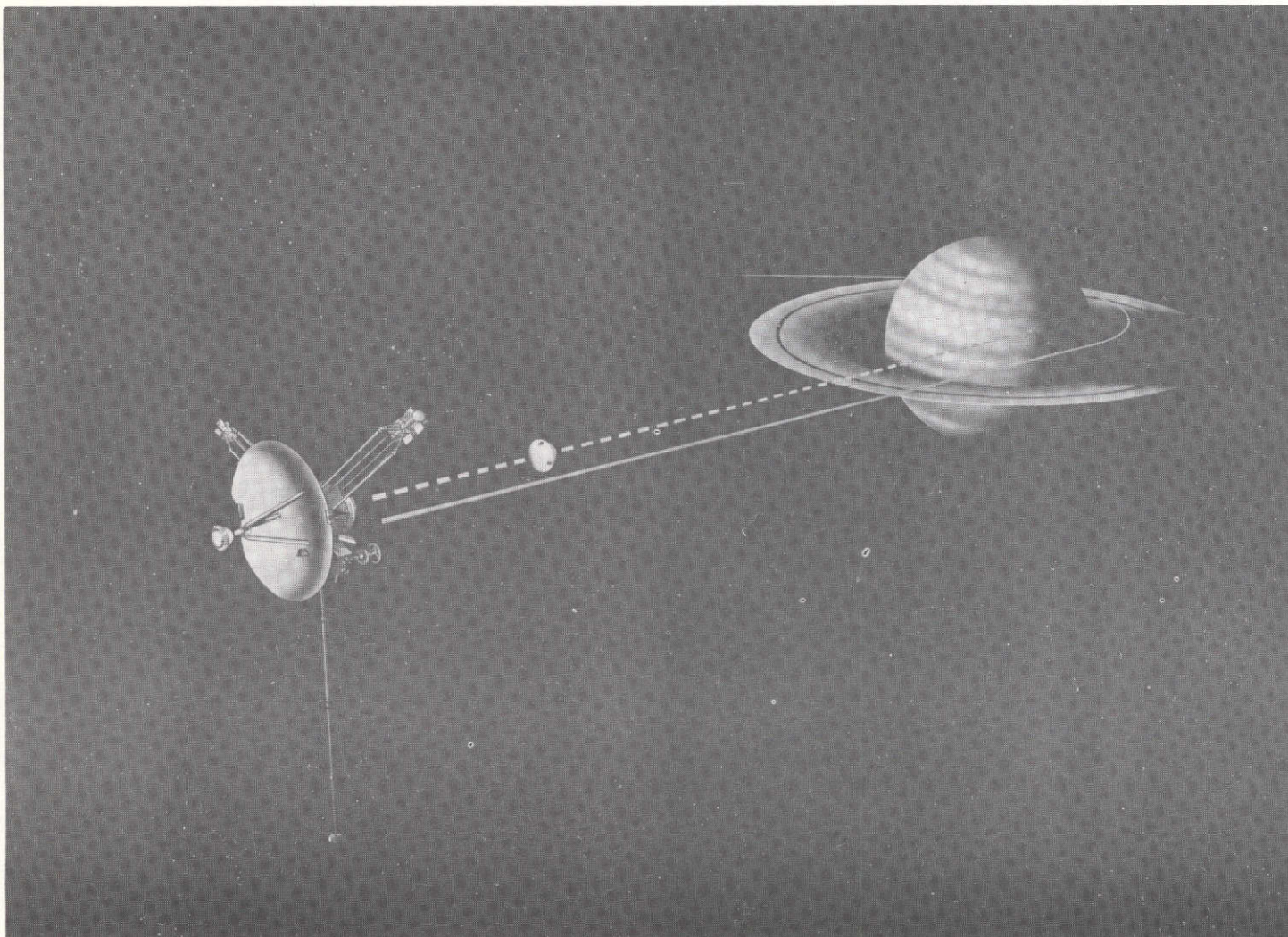


Figure 1-4 Pioneer Saturn Probe Mission

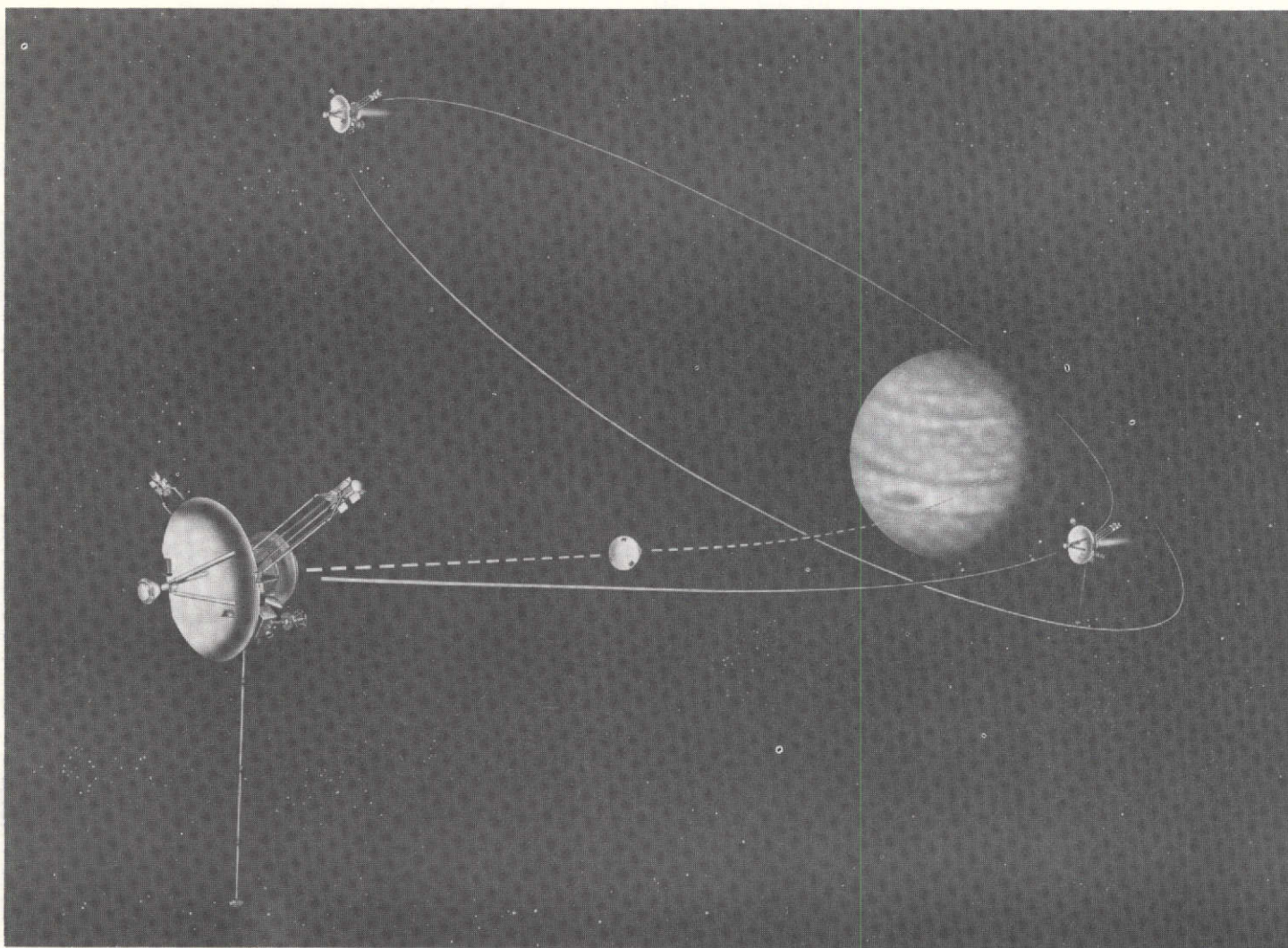


Figure 1-5 Pioneer Jupiter Orbiter with Probe

the payload that should be incorporated in the probe and this will serve as a guideline for the Phase B contractors.

Our current thinking is that this RFP, which would be competitive, would be released about July of next year and the procurement procedure would be similar to Pioneer-Venus. It would be open competition with two contractors selected to conduct a competitive Phase B and only the winners of the Phase B allowed to compete for the execution phase.

SECTION C

SESSION XI - SUMMARY ROUNDTABLE DISCUSSION

SESSION XI - SUMMARY ROUNDTABLE DISCUSSION

MR. SEIFF: We plan for the next two hours to try to sum up what has happened here during the two-and-a-half days of meetings. In view of Dan Herman's announcement at the outset that the planning for Uranus probe missions was becoming more firm in the sense that Phase B studies are to be undertaken, the panelists are going to each put a special emphasis on the feasibility of the Uranus mission and to comment on problems that they see remaining; things that should be done to solve those problems and to bring the technology up to the state where it is ready. If, indeed, it is not now ready, as I think it is in many of the sub areas.

We are also going to try to limit ourselves to something like five minutes each in the opening remarks on each subject area so that we can allow some time for interchange between the panel and the audience after we make the rounds. I think I prefer to let the panel's statements be uninterrupted in the sense of going from subject to subject until we complete all summaries. At that point in time, however, we are going to declare open house and we are going to receive comments from you. Or, if you would like to augment something that a panelist has said, or agree with something, or disagree with something he has said or raise questions, any of those things will be in order.

The order of the panel chairmen speaking will be the same as that used in the original program, with the exception that Larry Colin will speak for Ichtiague Rasool who had to leave. We will proceed on through the sequence, and we will close with remarks from John Foster and Paul Tarver, representing Ames management in the probe area and Headquarters NASA management respectively.

DR. LARRY COLIN: In case anybody is confused, I was not a member of the panel. All the panel members from the first session, Science Rationale and Objectives, left early and I happened to be walking down the hall and they asked me to summarize what they said. Since I didn't listen to all of them, I will make some comments of my own as well.

The point that they wanted me to stress was that exploration of the outer planets and their satellites by in-situ measurements is absolutely required if the major questions about the outer solar system are going to be answered. This is not to say that orbiter and flyby remote sensing isn't important. Certainly, they are important from the point of view of helping to understand some of the ground-based observations which have been collected over many, many years now. But there is no question that in-situ probing will be necessary in the long run.

Interest ranges over a wide spectrum of missions from simple missions of the kind that were mentioned consisting of simple temperature, pressure, and accelerometer instruments, plus the comparative atmospheric structure experiment (a payload which may be of the order of two kilograms), up to a full-blown entry probe mission of the order of the Pioneer Venus large probe mission, which contains about thirty kilograms of scientific payload weight.

The panel was very much interested in the proposal put forward by John Wolfe of a Pioneer-Jupiter orbiter dropping off a small probe which would be capable of carrying about ten kilograms of science. Ten kilograms fits nicely within the two-to-thirty spectrum that I mentioned. The experiments that are on the Pioneer-Venus large probe are, in fact, those which are in the primary payload including options mentioned at these meetings. Included are: (1) the atmospheric structure experiment (temperature, pressure, acceleration and, hence, density, of course, which results from these),

(2) for measuring the composition of atmospheres, both the mass spectrometer and gas chromatograph and their combinations, of course, are of interest, (3) for studying the cloud structure, the cloud particle size spectrometer and nephelometer, and finally, (4) for studies of thermal balance of the planets, devices like net solar flux radiometers and net IR flux radiometers would be very important in outer planet missions.

The question arose about payload commonality for Uranus, Saturn and Jupiter missions. The panel members definitely feel that trade-off studies are required immediately to determine the question of whether such commonality is desirable. Certainly, commonality sounds good, but it should be looked at from a scientific point of view for each of these outer planets and their satellites. As I understand it, NASA Headquarters has taken up this suggestion of a trade-off study and one will be set up this summer. Don Hunten will be organizing the summer study.

The panel wishes also, to endorse for outer planet science the basic approach which has been used for Pioneer Venus. That is, complete iteration and reiteration of the science objectives and instrumentation and spacecraft capabilities so that one can optimize and balance the scientific payload against the spacecraft design with the viewpoint of keeping as low a cost approach as possible.

John Lewis made a special plea in the area of composition measurements. Chemical analyses of the planets appears to be a relatively easy thing to do with the kind of instruments that are at hand today. The measurements of isotopes, clearly of importance in solar evolution theory, is the thing which is most difficult to do. The idea of a separate gas chromatograph and a separate mass spectrometer is certainly a desirable thing to have. The question of combining them, a la Viking, as a single instrument is something that he endorses for continued development.

Along this line, I would like to urge NASA Headquarters that they generally maintain a strong SR&T program for advance development of long lead time instruments.

Don Hunten cautioned that we should not overlook the importance of the upper atmospheres and ionospheres of the outer planets. After all, we do fly through them getting into the lower atmosphere, if for no other reason. But they are important for their own sake, and we have a ready collection of in-situ measurement devices: neutron and ion mass spectrometers, retarding potential analyzers, electron temperature probes, and airglow and dayglow devices, which would be very useful on outer planet missions.

With regard to Uranus, John Lewis stressed that it is the logical first choice; and the panel also feels it is the logical first choice for outer planet entry missions. They caution that the Pioneer 10 thermal results from the occultation experiment, which appear helpful from system design, are quite contradictory with regard to all other measurements that have ever been collected across the spectrum. They feel that all the conflict that has arisen makes it impossible to use the Pioneer 10 results as a basis for spacecraft entry designs in the future. Those results have to be understood if they are correct.

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MR. BYRON SWENSON: The Mission and Spacecraft Design Constraints panel had roughly ten major points that they would like to make. They divide themselves roughly equally into comments regarding navigation and comments regarding systems.

With emphasis on Uranus, the first and probably the foremost is a plea for an improved ephemeris of Uranus. We estimated that we could obtain this for a very modest expenditure; I believe about \$250,000. It seems that there is a real requirement that something be done along this line.

The second point also deals with navigation relative to Uranus. We have seen that optical measurements were required because of the ephemeris uncertainty of Uranus, but there is a question relative to the real-time processing of the optical measurements when you have something like a five-hour light time from Uranus to the Earth. And the software that goes into processing that type of data and the real value of that data is still in question.

The next major point is a systems oriented point relative to Uranus. There is concern by several members of the panel as to the system interactions and implementation of deploying a spinning probe off a 3-axis stabilized Mariner bus. The problems do not seem entirely insurmountable, but there are a lot of things that have not been investigated: tip-off errors, the implementation of the deployment; whether we should have a spin table; whether we should go to the difficulty of putting a spin table on the spacecraft; and so on.

The final systems oriented point relative to Uranus was the question of how much commonality should be carried in the probe design. Previously in the Saturn-Uranus probe studies where we deployed it off the Pioneer spacecraft, we did find that we could employ a great deal of commonality. But now introducing the Mariner into this and not only do we require commonality between the planets, but we must now require commonality between spacecraft. This

implies some penalties associated with the probe when flown on a Mariner.

For example, the frequency that was chosen for the Pioneer was 400 megahertz and I believe that 800 megahertz would be a more reasonable center frequency if you were flying off a 3-axis stabilized machine which had a highly directional antenna.

And, of course, a change in the communication system cascades itself right on through the system, and I am sure there are penalties here that we have not completely understood.

So we have the whole question of how much commonality is desirable and cost-effective.

Moving on to the Saturn and Titan missions, which were to be Pioneer launched, we saw that the capability to obtain a Titan intercept and the subsequent Titan occultation was indeed uncertain with the V-slit navigational sensor.

However, the point was raised that the tests that TRW has made on the V-slit have indicated a greater accuracy than was used in the calculations that resulted in the previous conclusion.

So it appears that if we are going to fly a Titan mission using a Pioneer spacecraft, there is more work to be done on the V-slit sensor to verify this greater accuracy.

For Jupiter probes, one of the major questions which has not been addressed sufficiently in the conference is the radiation hardening of the Jupiter probe. The probe does have to get in close to the planet by definition and it will encounter a great number of protons if the current models are correct. Some more light should be shed on this question with the Pioneer XI passage, which will give us much closer passage and a much better model of the proton belt.

A question was raised relative to pre-entry science data particularly at Jupiter. It was felt that the scientists - and I believe Don Hunten mentioned this - would eventually request pre-entry science. A dramatic impact is noted when you require pre-entry communications from the probe. I just want to highlight this because if you do put on pre-entry science you are going to really change the probe design.

And finally, there was a feeling that we should re-examine the deployment strategy for all these missions. They appeared to be common but there were slight differences. Nearly everyone is using deployment at 27 days prior to encounter. However, we saw some numbers slightly different from that, and it was felt that these factors do have some fairly sizable impact upon the systems, and we should, if we are going to have a common probe, standardize some of those factors.

MR. SEIFF: If I may exercise the Chairman's prerogative here, I would like to ask you one question. The suggestion that was made by Tom Croft, when coupled with the problem that was described by Donn Kirk, namely, the need for accurate initial conditions for reconstruction of the atmosphere - these seemed to couple together. He is proposing that the relative velocity between the probe and the bus be accurately determined prior to entry - after separation but prior to entry - and that the bus trajectory be accurately documented from its perturbation in flying by the planets which, coupled together, leads to a very accurate information, presumably, on the initial conditions for entry.

MR. SWENSON: I can't really comment on that. The only thing I can say is that the Mariner with its full optical systems will be able to deliver the probe to a much smaller entry angle corridor than the Pioneer can, for example, at Saturn. And this, too, of course has impact on the probe design and the question of how much commonality should be provided and the quality of the science you will get at Saturn versus Uranus.

MR. SEIFF: Tom Canning is next, to speak on the subject of the probe design.

MR. CANNING: Most of the things that I will comment on are concerned with probe system designs. There will be others talking about the sub-systems of probes, and I will try not to spend too much of my time on them.

With regard to the draft "10-Bar Probe" book that was sent out with invitations to this meeting, one point was emphasized through the study DYNATREND did with and for us, but may not have been amplified on adequately here; and that is in that book and in discussions during the last three days we see very different system designs to do the expected missions at Saturn and Uranus. This serves a purpose, namely, it tells you that either there is no single, unique design that will do the job, or these differences might imply that somebody is off on the wrong track in his design.

One of these designs was done essentially on the basis, "no-holds barred, re-package your payload, do everything necessary to design the system for the mission." The other approach which received a lot of attention was, "Here are a bunch of boxes and designed systems from a similar investigation, do this outer planet mission with them modified as little as possible." There were other minor differences in ground rules, but that really was the driver to produce the very different designs presented.

During this meeting all of the designs we have discussed in detail for the Saturn-Uranus entry and descent were unstaged designs, that is, they did not have a parachute stage to delay the descent at high altitude. One of the panel members urged, and I repeat his urging, that we really must not consider this to be a closed subject. We have to expect continuing evaluation

by the engineering and scientific communities on the impact and value of obtaining high altitude measurements. And an input to these trades would be the designs for staging via parachute-type systems.

Along the same lines of the continuing interest and influence from the scientific community, we clearly should keep a very active participation of a nucleus of scientists. During the formative phases of the project, we would like to know as accurately as possible what the scientific requirements are going to be when the mission is approved for execution. At that point, or shortly thereafter, we would like to have some way of finalizing on these science requirements, turning the scientists off, if you will, to let us get on with the system design in accordance with the requirements as have been established. And this always presents a problem.

In the middle of that problem is the establishment of priorities, or of principal goals in the case of a probe mission going to any of these planets. This usually manifests itself in the competition for weight, dollars, data, or any other measurable quality, between the probe that goes into the planet and the spacecraft which flies by. I think that this is a question which should be settled by the consensus of the scientists ahead of time; i.e. establish these priorities, and then stick to them. I can see grave difficulties and costly perturbations to a program if those priorities are not carefully settled in advance.

Another comment that came from this discussion was concerned with schedules and that we should do our best to pace the program very carefully in accordance with what we are able to do. That is, to base the next program, or perhaps the next two programs, on what we are quite confident we can start out to do right now. Perhaps, even restrict these programs to things that we know damn well we can do. The danger of that approach, however, is that we would be neglecting the long-distant program; obviously, in this

case a Jupiter probe mission which presents a major step in difficulty from the other outer planets.

We certainly would like to consider the possibility of what one might call a revolutionary advance for that program, even though we don't demand or we would not even intend to use such advances for earlier programs unless they came along very rapidly. An example of this advance could be the continued development and availability of a characterized reflecting heat shield.

Another point should be made: several speakers indicated that Jupiter entry is now so much easier with the improved ephemeris, improved navigation and so on based partly on Pioneer 10 data. This discussion was very optimistic. On the other hand, not sufficiently emphasized is the point that the heat shield of this Jupiter-entry vehicle does not change much. Even with shallow entry, the probe is going at 50 kilometers per second and has to be slowed. The heat shield will remain to be the design driver.

My group then discussed the philosophy of the control of system design for long term missions, and this is in the area of the reliability of the hardware produced. We typically characterized the hardware that we have used, the subsystems and the total systems, by reliability numbers. Analyses should be continued with regard to the cost-effective approach to reliability for long-term missions: redundancy of equipment vs. high reliability demonstration projects; reliability analyses, failure analyses, and the examination of the consequences of failures. The JPL approach to this subject should be examined since it apparently works well as demonstrated by the Mariner-Venus-Mercury; Mariner X mission. There were equipment failures and yet the mission was a fantastic success.

MR. SEIFF: The critical areas of heating estimation and heat protection will be covered next and Dr. Walter Olstad will address the first of those subjects.

DR. WALTER OLSTAD: From the point of view of entry aerodynamics and heating, being asked to focus on Uranus really doesn't restrict me at all because we know so little about Uranus. What we know about the atmosphere is that there is some hydrogen in it and there is some methane in it. And if we design for what is now considered the worst case, the entry in terms of heating rate is about as severe as the nominal Jupiter entry. Thus, if Uranus rather than Saturn or Jupiter is chosen as the first target for an outer planet probe, the problem of entry heating is not greatly simplified.

And that brings up the first point. We need a good handle on the range of possible atmospheres. We'll let someone else worry about what the probabilities are but let us know what the range of possible atmospheres are and we'll exercise our predictions over that range. Then the decision makers can work with those numbers as they will.

An interesting feature about outer planet probe missions is that we are going to have to rely much more heavily on analytical and computational predictions without backup experimental verification than ever before unless we undertake a flight experiment which could be a very costly thing. So we need to assess the risks, and we must assess them quite carefully. This is something we should get on with right away.

Now, let's look at our ability to predict heat transfer for probes entering the atmospheres of the outer planets. Most of the analyses have been confined to the stagnation region. They are quite sophisticated and we feel quite confident we can come up with a conservative number and one that is not so far out of the ball park that you are really compromising probe design. However, we have no real experimental verification. Any verifi-

cation we have is a partial verification under conditions much less severe than required.

As we go away from the stagnation point on the probe, things get worse. At the present time, we have just a few analyses, a few analytic tools available and there are some serious deficiencies in these tools. These deficiencies have to do with things like predicting transition, determining turbulent heat transfer and determining the chemical state of the ablation products. These deficiencies are going to remain because the only way we can get at them is experimentally under the same conditions the probe will experience. It is not easy to extrapolate from experimental experience when you are talking about transition and turbulence. What we do now is take a lot of data and fit curves through it. The curves are not based on any physical reasoning so when you try to extrapolate a long distance from the original data base you can be badly misled. There are plenty of examples of just this sort of improper extrapolation throughout our short history of entry vehicle design.

So we are going to be faced with considerable uncertainty, and it is important that we try and quantify the uncertainty so that a proper assessment of risk can be made. Furthermore, we need to improve the analyses in the down-stream region as much as we possibly can. We are working at that right now.

If we go farther back on the probe to the probe base area, again we depend almost entirely on experimental numbers for base heating. That is not anything that is really going to make or break a mission, but there is a lot of area back there and the heat shield weight is significant. So, again, I think we are faced with an uncertainty and it is important that we try and quantify that uncertainty.

In general with regard to heating, if we find after trying to quantify uncertainties, that the risk looks pretty large, it might make sense to try and get some experimental data. The only

way I know to do it now is a flight experiment, and that could be very costly. So the risk-cost trade off is a very serious one.

It is interesting that, for the Viking mission, where the heating is not very severe and where ground facilities are adequate, the Viking people are putting a 1.5 factor on all of their heating predictions. If we start putting a 1.5 factor on heating predictions for the outer planets, we are liable to put ourselves out of business. And yet, the uncertainties are probably going to be a lot greater for these outer planets than for Mars. So, again, it is extremely important that we try to quantify these uncertainties.

In addition, we need to perform a number of parametric studies over the range of possible atmospheres. All we have looked at are a small family of blunt cones and Apollo shapes and the so-called model atmospheres. Furthermore, most of these parametric studies were performed some time ago. Now our prediction methods, while still far from adequate, are much improved. Perhaps through proper studies we can identify a better configuration.

With regard to aerodynamics, stability, of course, is an important problem. We want to know what orientation the probe is in at all times. We feel quite confident that we can guarantee a stable design although there are some problems having to do with large blowing rates, axisymmetric ablation, things of that sort, but they don't seem to be particularly serious. They are problems we are going to have to work out, but will not require any unusual effort.

With regard to performance, the Viking people say that they would like to know their aerodynamic coefficient within five percent in order to get good information on reconstruction of the atmosphere from accelerometer data. Here, again, I think with some work, with some studies in facilities that we already have, complemented by some analytical work, we can probably achieve that level of accuracy.

MR. SEIFF: Thank you. Inasmuch as there were very few results given in the meeting on heating on the probes for Uranus, I took the liberty of looking in some old publications that are in my office to get some numbers and I saw in a study that Mike Tauber did about four years ago a value of the mean heating rate of six kilowatts per square centimeter for a body somewhat blunter than the ones that are now being considered.

I think one of the McDonnell-Douglas people showed values equivalent to twenty-four kilowatts per square centimeter. These values are, by comparison with those that have been computed for Jupiter entry, quite modest.

DR. OLSTAD: But if you look at the worst case, the radiative heating rate goes up to fifty kW/cm² and that coincides with a nominal Jupiter entry. Now unless we learn that the worst case is highly improbable, we must design for it. Furthermore, we don't really know that the current so-called worst case is the real worst case.

MR. SEIFF: What does that worst case correspond to?

DR. OLSTAD: That is the cold dense atmosphere and a steep entry.

MR. SEIFF: What does that imply with respect to sixty percent helium?

DR. OLSTAD: The cold dense atmosphere assumes 60 percent helium by volume.

DR. NACHTSHEIM: The heat protection group organized their work into an assessment and recommendations and they also made an observation focusing in on the question of Uranus.

As far as the assessment went, there were five points that were made. The first one had to do with the characterization of carbonaceous heatshield materials. The group felt that the thermochemical prediction of graphite and carbonaceous material was predictable. Particulate removal could be handled within the range of our experience by applying a design factor. Two different studies have used a design factor of 1.3.

The third point under the characterization of carbonaceous material was that there was no agreed-upon particulate removal mechanism.

The second main point made in the assessment was that the silica-silica heatshield needs further characterization. However, it was pointed out that there is a wealth of knowledge on the convective performance of pyrex and quartz heatshields that dates back to the 1960's and that many missile radomes are made out of this material. This information should be looked into.

The third main point of the assessment was that all possible mechanisms of ablation and intense heating are not known at this time. They are undefined.

The fourth point under the assessment was that present facility capabilities exist to verify heatshield designs, on a small scale of course, for Venus and that such capabilities do not exist for the outer planets. In other words, Venus is the limit of our capabilities with existing facilities, at the present time.

The fifth and final assessment point was that our flight experience with radiation present is the Apollo experience.

There were six recommendations. The first dealt with carbonaceous materials. Under this topic, one point is that we should characterize carbonaceous materials at the highest heating level possible. Second, we feel that we should increase the laser power so that we can get larger heating areas. The third point under this main topic of carbonaceous materials is that we should combine the laser with an arc jet and get combined heating. The fourth point under carbonaceous materials would be that we should exploit graphite performance, and we should start studying the graphite-insulation system as a heatshield. Graphite by itself is not a heatshield material. It requires an insulator. Another possibility is to look into the concept of a hot bondline.

The second recommendation deals with silica-silica heatshields. There are several points under this. One is, development should continue. Second, the silica material should be exposed to the solar spectrum at high heating rates. There are some facilities that utilize the sun with huge arrays of reflectors to get heating levels on the order of six kilowatts per square centimeter. The silica material should be exposed to that environment. Third, another suggestion was to design a material to reflect laser radiation. In other words, the technology is understood to reflect visible radiation. Since our intense source of radiation is the laser, you should be able to demonstrate reflection at 10.6 microns if you understand the problem well enough.

The third recommendation had to do with a design philosophy. It was the consensus that we should exert every effort to verify heat shield design in ground-based facilities before flying a mission. That is the recommended design philosophy.

The fourth recommendation had to deal with the engineering flight experiments. We feel that these should be studied in terms of earth entries, looking at the Langley proposal of a rocket-launch experiment. And in the 1980's, possibly a shuttle-launched experiment should be considered.

Also, in the way of an engineering experiment a planet should be considered. What we suggest is to put the question the other way around. If you could optimize the heatshield design to go to Jupiter, do so; and then ask yourself what science could you take along with that. This would be a feasibility study to determine the engineering feasibility of sending a probe into Jupiter. The Jupiter entry engineering experiment would be comparable in cost to earth entry experiments. This is not unlike the Apollo experience. Before we put a man in the Apollo vehicle, a whole class of vehicles were flown. This suggestion says, "Let's build an engineering probe with modest science, demonstrate the feasibility, then have the elaborate science." There, we would be simulating everything in full scale. It is a serious suggestion.

The fifth recommendation is to continue development of the giant planet arc, and this is being driven by a Jupiter 1984 launch.

The sixth recommendation is to accelerate development of the giant planet arc, and this would be driven by the Uranus 1979 launch. At the present rate of development, it could not assist that mission.

Then, finally, we made an observation that the life style of the NASA entry technology personnel will change if the support of the Uranus probe increases for the 1979 mission. The personnel currently at Langley and at Ames are only skeleton crews compared to that which will be necessary to support the Uranus mission.

MR. SEIFF: The subject of communications is equally critical because without communication all is for naught. So, Terry, would you give us your appraisal of that situation?

MR. TERRY GRANT: I think the first item that can be derived from our splinter meeting is that, by virtue of the absence of discussion, we should conclude that there were no problems uncovered in the Probe-to-Bus communications for a Pioneer Saturn-Uranus mission with the present science requirements. In other words, the baseline design with the ground rules that were originally given does not appear to have any technology problems associated with it. If new science requirements are added, however, the baseline design will have to change. The first requirement and the one which was discussed most was the requirement for pre-entry transmission. The consensus at the splinter meeting was that the communications required for this could be accommodated, but that it is impossible for us to assess at this point the complexity of that communication system, or the costs related to it, until we have some more details about this requirement.

For instance, we really need to know what kind of frequency stability is required for pre-entry transmission, since one of the criteria for an experiment using pre-entry transmission is to measure the electron density along the propagation path.

Also, we need to know what data rates are required. If it is postulated that there is a small amount of science and it has a low data rate, this pre-entry transmission might be relatively easy to accommodate.

Of course, an important parameter of pre-entry transmission is the time required. The transmission time and the data rate are more related to total system requirements than to communications. Once you build a transmitter it can provide transmission time in direct proportion to the battery and thermal capacity of the probe.

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That was one point that we wanted to emphasize; that the pre-entry transmission is also a systems requirement and that it would impact the systems design as much or more than communications. Therefore, trade-off studies of the complete system are required in order to come up with an efficient new baseline design.

The other point with regard to science requirements was that there seemed to be an indication that additional scientific data would be required during the descent portion of the mission. This, again, would impact the baseline design for communications.

MR. SEIFF: What, specifically?

MR. GRANT: Well, I was thinking specifically of the interest in the gas chromatograph and I can see that the data rate originally defined is likely to be considered sparse if the gas chromatograph is an added instrument.

I point this out because while the baseline design accommodates the relay link at 44 bps, it doesn't do that with a large amount of margin. Furthermore, the baseline design cannot be extended very far to accommodate higher data rates by simply adding power, for instance. It will require extensive re-design if we require much higher data rates.

Going on to particular comments relative to the Uranus mission with a MJU probe, it is important to realize that the commonality considerations in this baseline design keeps it from being optimized for a Uranus mission, particularly for a Uranus mission with a Mariner-Jupiter-Uranus/probe.

First of all there is no turbulence proposed in the modelings for the Uranus ionosphere, or atmosphere. Therefore, we might achieve more efficient communications by going to a phase-modulated signal rather than a frequency-modulated signal as we have now.

Secondly, with the Mariner three-axis stabilized vehicle, the use of the pointing antenna would make a higher carrier frequency more optimum; I think Tom Canning or Byron Swenson pointed this out earlier. We recognize that a commonality of communications design for outer planet entry probes does make the design sub-optimum for a Uranus mission.

Another point that came out perhaps more rapidly than we would have liked was one that Kane Casani brought up in another presentation. That is, there are conflicts between the flyby bus and the probe priorities and they showed up in the papers that were presented; particularly, in the paper that was presented by Paul Parsons. There are a few interface problems that show up immediately. One is that the optimum probe antenna beamwidth for the presently-envisioned Mariner-Jupiter-Uranus trajectory is wider than the probe beamwidth that we have in our baseline design. This problem is not inherent in the Uranus mission but it is inherent in the considerations that were given to the Uranus trajectory. I believe the trajectory was set up so that the bus science would be free to operate without interference from probe transmissions during the closest approach to the planet and, therefore, the probe communication range and aspect angles were non-optimum.

Another interface problem relates to the allowed storage on the bus for probe data and the rate at which probe data can be relayed in real-time to the Earth. If bus storage up to a million bits and real-time transmission of 264 bps can be allowed, an efficient code can be used for the relay link by taking advantage of a complex decoder on the ground. However, if the storage and transmission rates are appreciably less, decoding on-board the bus may be required, resulting in more weight and cost for the probe communications subsystem.

The other factor that requires a technical decision on the interface is whether or not some amount of antenna steering should be provided for the relay receiving antenna on MJU. The current baseline for the MJU bus is to have a fix-mounted antenna. So here again we have an interface where, obviously, from the bus point of view a fixed antenna is desirable but if you look at the overall mission priorities you might want to allow the antenna some degree of mobility in order to optimize the relay link.

The last factor is one that goes along with what I said earlier, that the baseline as it now stands does not have much margin for increasing its capability. There is a possibility, however, that within the next year further information on the turbulence models for the outer planets, and also on the expected modem and coding performance, could conceivably improve the link capability over what we now use as our baseline. I think that there will be new information incurred in the short run that will bear on the baseline design for communications.

MR. JOEL SPERANS: The Science Instruments Group, by contrast to what I have been hearing the last few minutes, tended to take a very conservative point of view with regard to the outer planets missions.

We concentrated on the baseline programs and I think at this point we would have to say we will give Terry Grant very few communications problems of the sort that he suggested.

The opinion in general was that we should concentrate on doing one job and doing it well, and that the baseline job in this case is the lower atmosphere. From that it followed that we felt that by a combination of atmosphere-structure experiments and a combination of mass spectrometer and gas chromatographs, both of which are in a fairly high state of development at this point, we could do a pretty effective job with the payload capabilities that we have available to us today.

We did consider a number of specific problems in areas in which more money and more effort should be put. In general, they are relatively minor. Certainly more emphasis needs to be put on the study of the problem in operating in a helium environment and pumping helium in the mass spectrometers. These studies are being funded now, are going on and appear to be very successful. The consensus was that this did not represent a great problem in the long run.

An issue that has not had much emphasis put on it so far is the question of survival and operation of some of the basic instruments after a shelf life of seven years. Most of our instruments are ready to fly but they are not necessarily ready to fly all the way to Uranus. It is going to take a while for us to be sure that after seven years of sitting around on a spacecraft,

or on the shelf, these things will operate in a way in which we can understand them. Again, these aren't expensive tests but they are tests which I think should be initiated very quickly.

I think the most significant outcome of our discussion was the emphasis that we all place on the need to put more time and more consideration into the application of the gas chromatograph family of instruments into the outer-planet instrumentation.

We would like to enthusiastically endorse the removal of the stigma of the so-called "ten-bar probe" that we see on a lot of the documentation which seems to be coming out of Ames and a lot of other places in the last few years. In the view of the instrument people, this is not a ten-bar probe; it is an outer-planets atmospheric probe and we will get information as far down into a planet's atmosphere as the spacecraft can provide us with communications.

There are one or two other minor tests that we would like to see; that we would like to endorse: such as the trade-offs between pressurizing the entire vessel or spacecraft versus trying to build instruments that can operate in unpressurized atmospheres. These are things that should be undertaken and will be undertaken in the near future. I don't think they represent large investments of money or talent.

Other than that we felt that the basic instrumentation for the lower-atmosphere science was in pretty good shape. Certainly by the time the instruments fly on Pioneer-Venus we will be in very good shape in those areas.

Because of its composition, this particular group, felt that it did not really have the mandate to consider to any great extent the apparent lack of emphasis to date on the middle atmosphere measurements. Larry Colin brought this out quite

effectively in his opening remarks and I am sure Don Hunten too would emphasize these to a great extent. We haven't paid sufficient attention to the problems of making measurements in the so-called middle atmosphere.

One possibility for doing these in a low-cost way is the shock-layer radiometer or some derivation of it. This instrument is reasonably well-developed and reasonably inexpensive, but again, we did not feel this to be within the province of our particular group. Although we are not endorsing it strongly at this point, we feel that a lot of serious thought should be given to considering the shock layer radiometer as a fairly low-cost, easily-accommodatible addition to the outer-planets payload.

I think that about concludes what we discussed.

MR. VOJVODICH: Did your instrument group address the operational question of penetrating heat shields and getting a resultant clean sample of gas to analyze?

MR. SPERANS: Yes, we did. We discussed that at some length. The reason I didn't mention it was that it did not appear to be a problem. We discussed several options: several ways to do it. In general, if we can poke a big enough hole through the heat-shield and get a decent size sample to carry enough gas inside to where the gas chromatograph and/or the mass spectrometer can operate on it, the problem of working through the heatshield doesn't appear to be formidable.

MR. SIEFF: Okay, thank you very much, Joel.

MR. SEIFF: The next technical category is that of Special Subsystem Design Problems which, in our meeting here, turned out to be primarily sterilization and radiation effects. Ron Toms of JPL will give us the summary group report.

MR. RONALD TOMS: Well, in fact, the session we had did not include a splinter group meeting. We had such a diversity of topics that it didn't seem particularly appropriate to break out into a splinter group.

The particular topic of planetary quarantine is one, of course, that has been worked on a great deal. We started off by hearing the ground rules of the game that we are supposed to play. Next we heard about the way in which we would do quarantine for the outer planets, and the effects on probe design. Then we heard a horror story of what Viking has to do to meet the kind of requirements imposed upon Viking. We don't know the cost of that; and Viking is not, in fact, making an effort to keep the costs of providing planetary quarantine as a separate, recognizable item.

I think we are a bit comforted though by the hope that heat sterilization requirements of outer planet probes will be unnecessary. Those of you who were here on Tuesday morning and heard Dan Herman's statement of his position on this heard that (for the time being at any rate) in our mission designs, in our cost estimates, and in the way we plan the mission we won't include planetary quarantine, even though we will also do studies to find out what it would cost and how it could be implemented.

On the radiation environment and its effects, I think I could summarize best by saying that the MJS spacecraft is solving the problem for the MJU mission of what you do about flying past Jupiter to carry a probe that would go on an MJU mission to Uranus. A seven-year flight to Uranus, flying past Jupiter, would go by at $12R_j$ which is a fairly modest radiation dosage compared with some of the cases that MJS itself is looking at (which go all the way in as close as $5R_j$ and pass out to 8.5 or 9.) So as MJS solves the

problem it will, in a way, get solved for Uranus. Nevertheless, the probe itself has to be designed to meet the particular environment.

The Jupiter entry is another problem, and a probe that goes into Jupiter will have to be designed to meet the environment which by then we hope will be much, much better known not only from the later Pioneer data but from the MJS data itself.

The other two topics we tackled were battery life and thermal design: battery life for a seven-year class of mission and thermal design for the kind of conditions met in going out to the outer planets. Some significant problems were stated, and some adequate-looking solutions were discussed and given quite a good airing here.

I have a couple of comments on the MJU mission itself. It seems to me that it clearly is time to open up the probe-science question and then to optimize the probe design for the Mariner as a probe carrier. The other item is that I feel it very important that you all recognize that the MJU performance was not well reflected in the draft document that was sent out to everybody. I don't want anyone to go out from here thinking that MJU mission carrying a Uranus probe can only be flown off the shuttle, so that won't be happening in 1979. The performance capability is available with the Titan, and corrections of the document will be made before it is used in presentations to the SSB, OMB and Congress.*

* (Updated information has been received and included in the August, 1974 issue of the document "Atmospheric Entry Probes for Outer Planet Exploration - A Technical Review and Summary" Ed.)

MR. SEIFF: Now that brings us to the cost session, which was the most recent one this morning, and Nick Vojvodich will summarize that.

MR. NICK S. VOJVODICH: Since the cost session was held so recently, we changed the order around and our splinter group actually met before the general meeting. We had about an hour and all the cost session speakers sat around the table and dissected program cost estimating from the standpoint of whether it is a black art or whether it is a science or indeed a combination of the two. I have some random thoughts that I jotted down during the splinter session that might be of general interest.

One of the reasons we had so many questions at the end of the open session presentations is that, as Steve Georgiev of DYNATREND was saying, in technical areas some people always feel uncomfortable; however, when it comes to cost, everybody is an expert. That observation was reflected in both the nature and extent of the comments and I hope we get into this cost area a little bit more as the discussion that is to follow this round-table summary develops.

One of the critical points that was made during our splinter discussion by all speakers was that low cost methodology must truly be specified at the beginning of a program. That is a procedure must be set up to: monitor and to control the costs; reduce the required paper work; and minimize tests and development costs wherever possible. Namely, achievement of low cost goals is not obtainable by applying cosmetic changes to a "business as usual" approach.

Another important point that was brought up is that inherent in the traditional way of looking at the cost-weight sensitivity of a subsystem namely, the cost of subsystems grow with weight - is that the functional performance also usually goes up.

We are in a situation now, though, that if a system has excess weight capability, and if, in fact, low cost and design-

to-cost are constraints, fix the performance requirements and take advantage of the weight contingency to realize the cost savings. This is opposed to the historical approach of letting somebody come in and say, "If I could only get two more bits of data," or, "If I could only have one more sensor or more dynamic range capability." Probe entry systems are not linear so that a small change in one subsystem tends to perturb the system as a whole, and you have an uncontrollable growth situation. As somebody once said, "sometimes the spacecraft is growing so fast that one wonders if the launch vehicle will have enough boost capability to get it off the ground."

The question, of course, of inheritance was addressed during all of the talks and it is at this point that we get a direct interplay between technology and cost in some of the areas we were discussing earlier. John Niehoff of Science Applications Inc. emphasized that programs which push the frontier of technology run the risk of encountering potential problems that may require a substantial number of additional tests and thereby become susceptible to significant cost overruns. Therefore, early attention to technology development and assessment and working the identified problems by doing the appropriate SR&T, can significantly impact the program cost, schedule and technical achievement.

Specifically, in the area of the heat shield, we recognize that there is a quantifiable risk that one can handle by application of a conservative margin of safety to the design. Regarding this point, Fred Bradley from McDonnell-Douglas made the observation based on his participation in a number of previous successful flight programs ranging back to Gemini and Apollo, "we've never really started a program where we have had all the technology in hand. We have applied engineering judgment where appropriate and used some of the available weight contingency as a factor of safety and thereby eliminating the necessity of having to go down to the last five percent or ten percent in

either the prediction or the simulation of the heating environment." I am sure that we will get into a discussion of that philosophy a little bit later.

From the standpoint of the track record of these costing models that are used in project funding estimation, it appears that by and large they generate predictions that have been found to be within twenty-percent of the actual costs. That was more or less an established goal of these cost models. But if we are really trying to do business in a new way, one wonders whether we should continue to use these cost-estimating models which essentially are mirrors that reflect the past. So this point was also brought up, that we've got to make sure that the cost estimates are realistic, especially the early ones.

I want to close by emphasizing my last statement. That statement coincides with a comment that Dan Herman previously made at the end of the meeting; namely, the early cost estimates, made in a phase zero, or pre-phase A, are most often the costs that both the program manager and the contractor have to live with. It is, therefore, extremely important that the cost people interact with the technical people particularly during the formative stages of a program and get a good, solid, definition of the system so that unexpected surprises are not encountered as the program develops.

The key word here to categorize this aspect of the cost situation is one of credibility. We have to develop a funding estimate that is not only credible but one that is also realistic in terms of existing technology.

That's the end of our cost-session wrap-up. It was a bit disjointed but I feel that it accurately reflects our thoughts. I am hoping that John Niehoff, Fred Bradley, and Bill Ruhland will add to the follow-up discussion.

MR. SEIFF: Now we come to John Foster who is in the enviable position of not having heard the meeting, but being asked to comment on its conclusions.

MR. JOHN FOSTER: I have two points I would like to make from the Ames' management standpoint and, particularly, from the Pioneer view point.

The first point is that we are interested in probe technology because we are interested in future probes. As you know, we are in the middle of the Pioneer-Venus probe mission and Ames and JPL are both looking into outer-planet probe missions. I would like to clarify at least one point on that. There was a recent article in one of the aerospace newsletters that said that NASA plans to do all their outer planet probe missions using the Pioneer Venus spacecraft. It is not true, for a number of reasons. First of all, the Pioneer-Venus probes are 100-bar, hot probes. It is a different mission than the one that we are talking about, which is around ten bars, and at different temperatures. I want to assure all contractors that this is still an open ball game.

The last thing I would like to say is that it is my observation that the time is ripe to look forward to the outer-planet probes, and particularly the Uranus probe. Certainly JPL and we, and I am sure many other people, are very, vitally interested in this coming mission.

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MR. PAUL TARVER: John Foster narrowed his comments to three points and I am going to narrow mine to one. If I may, I'm going to deviate a little bit from the chairman's admonition to stick to Uranus.

This is something that has rather strong programmatic implications both as to mission sequence and our SR&T planning for the whole series of outer-planet-probe missions.

You probably noticed in the mission model that Dan Herman showed that the Jupiter-probe mission is scheduled for 1984. This decision was made with the advice of the scientific community, not because it ranked below the other planets in terms of science interest but on the basis of when it was estimated that we'd have the technological capability to do it. This estimate was based on our prior estimates of the nominal or the less favorable Jupiter atmosphere and ephemeris accuracy that was available.

Now, as a result of Pioneer 10, the improvement of the ephemeris and the possibility of a warm, expanded atmosphere, in some respects opened a Pandora's box, which should be opened. There is no complaint about that, but undoubtedly we are going to get pressure to bring a Jupiter-probe mission off sooner. We need to have some better facts, some better assessments than we have now as to whether this is a practical thing to do.

The present structure of outer-planet-probe sequences, is based on the development of a common Uranus and Saturn probe with the first Uranus probe on the MJU, followed by a Saturn probe later.

The question now arises, can we do a Jupiter-probe mission using Uranus/Saturn probe technology? If we can, then I am sure many people will want to do a Jupiter-probe mission sooner.

So, I am making a plea for this: that we do what can be done to get as much narrowing as possible of the uncertainty estimates in the environmental parameters that are involved.

Then, based on that, an assessment in as much depth as we can, of the feasibility of doing a Jupiter-probe mission with Uranus-probe technology. And deriving from that an assessment of the risks involved if we attempt to do a Jupiter probe mission that will employ common technology with the Uranus/Saturn probe.

Obviously, this has to wait for further verification from Pioneer 11. But, when that is available, then I think we need to do the studies to attempt to quantify insofar as we can the risks that would be involved so that we can make the necessary decisions whether it is feasible to move up the Jupiter-probe mission.

MR. SEIFF: We have now reached the point where we are ready to involve the audience in the discussion. We have gone around the table and now is there anyone out on the floor who would like to raise any questions?

MR. NICOLET: I would like to address this comment to Walter Olstad about the heating between the worst case of Uranus entry and the Jupiter nominal situation. If you were comparing the maximum heating levels which occur at one point in time as you enter, in fact I think that is comparable to the maximum heat levels for the Jupiter entry, but that is only a fair comparison. If you look at the Saturn warm entry to explain the worst flux, which is maybe only 5,000 kilowatts per centimeter square, the requirements on the heatshield are almost as severe as for the Uranus probe with its terrible helium content. The point is that the time requirements are there and they are very important; and for either Uranus atmosphere, the heatshields are only slightly different and the requirements on the heatshield are a lot less in the Jupiter case.

(NOTE: The following notation dictated by Mr. Nicolet after the round table session).

****My** comment was with regard to Walter Olstad's analogy between the most severe Uranus entry heating condition and that for the nominal Jupiter entry. The comparison was between the maximum heating levels which would be encountered at one time on the trajectories, that is the maximum heating levels for an entry. That is not an entirely appropriate comparison as the time integrated heating pulse more directly bears upon the required heatshield thickness. For example, the entry into the Saturn warm atmosphere encountered a heat flux no higher than about 5 kilowatts per centimeter square. However, the heatshield required for that condition was almost as great as that for the Uranus cold dense entry where the maximum heating levels were roughly 50 kilowatts per centimeter square.******* (End of dictated notation.)

DR. OLISTAD: There are two aspects to the problem, and one is the total heat load. And certainly, for Uranus, it is considerably less than what it would be for Jupiter and, as you say, a shallow entry into the Saturn warm atmosphere is a severe case. The other aspect is the heating rate and we don't know what is going to happen to a heat shield when it is exposed to very large heating rates. We aren't able to produce these conditions in ground facilities at the present time, and until we have some experience, heat shield behavior will remain a matter of particular concern. So the heating rate is an important factor. Current estimates of heat shield weights for outer planet probes are based on the assumption that the heat shield materials will respond to heat loads in the same way the Apollo heat shields did. This is a very crucial assumption. If we find that heat shield materials respond in a different way to large heating rates than to the smaller rates of current experience then our estimates of heat shield weights may be seriously in error.

MR. SEIFF: One comment that I think Nick made was very interesting to me, and that was to point out the fact that on many of the earlier missions that we have undertaken the uncertainties have been very great.

When John Kennedy stood up in 1960, or whatever year it was, and said, "We shall go to the moon," there was nobody around who really knew that we were going to go to the moon.

So uncertainty in the projections of future missions is by no means a new thing. And, really, what usually happens is that people rise to the challenge. Once the planning is made definite, people rise to the challenge and they do the job that has to be done. I would fully expect the same thing to happen here.

MR. SEIFF: Ron, you have some remarks?

MR. TOMS: I wanted to raise some points where I think the Mariner mission has really not been well understood by this group. In particular, the question of what you do about communications. Now, in flying the Mariner spacecraft and being able to use a body-fixed antenna with an extra five or six db gain, the first thing that you can use the extra db for is to move from the dark-side entry to the light-side entry, which is what the atmospheric physicists particularly want. Flying around on the right side of the planet instead of the left side also allows you to get a very high escape velocity from the solar system, which is what the inter-galactic investigators want.

The next candidate for using some of that db gain is to not have to fly by at some specially-optimized flyby distance from Uranus but to have flexibility, for example, from about 2 to 4 R_U .

And the third thing you can use it for is a somewhat higher data rate, if there is any need on the part of the scientists to increase the data rate above the one that's now being looked at.

A fourth thing, then, is that of taking the probe data a little earlier in order to get better pictures. That doesn't mean to say that one can't take the data at the same time as was previously planned, but if you have the extra db gain then you can optimize a best combination of probe data and picture data.

A fifth way to use that extra gain would be just to lower the probe power by perhaps a factor of two. So there are all those candidates.

Then, there is another way of increasing the db gain in this data link and that is to move to a higher frequency. There is no suggestion that Mariner wants a higher frequency. It doesn't need it, but it would be another point of gain that one could make

to move up to 860 kHz or thereabouts.

Now, there were some remarks, too, that puzzled me about whether or not we knew we could deploy a spinner from a three-axis stabilized spacecraft. Certainly we can. There are a couple of very good designs; both of them adequate and both of them quite inexpensive and not costing us very much in weight. There were some numbers in the handout (the Ten-Bar Probe document) which talked about it costing 70 kg to be able to incorporate the probe on the Mariner. It must be a typographical error. It only costs about 10 kg for all the additional things that one would want to do to the spacecraft, including putting the relay-link antenna and receiver on it, plus about 25 kg of propellant for the additional maneuver. The tip-off conditions have been looked at and they are relatively modest. We are even looking right now at a way of getting very, very close tracking of the probe by simply turning the imaging system on to the probe as it leaves the spacecraft. There we would get a very precise way of monitoring the probe trajectory and extrapolating to accurate entry conditions.

I want to take issue with something that Tom Canning said, on a quite different topic. Tom, you said, I think, that you wanted the Science Advisory Committee to be turned off and to have a frozen position on priorities (when the program begins). That would be a disaster for a mission of this kind.

MR. CANNING: I was just trying to avoid those major surprises once one starts the program.

MR. TOMS: I think that is right, but you see there is always the danger there that we either fly the wrong mission or we propose to fly the wrong mission and get turned down because it is the wrong one.

And I think that continuing the Science Advisory Committee at full strength all the way through, is important. No more messing around with AMDO's and all that sort of thing.

MR. CANNING: On the other hand, if you want to control costs, as we are going to have to do, if we make major changes on the demand of the system part way through a design, well, I don't have to state the obvious.

MR. TOMS: No, but we must always be ready to.

MR. CANNING: Even that is expensive.

MR. JIM HYDE: I have a comment. There is a very specific thing to be considered here. For some time Ames and a number of industrial contractors have been studying the probe that we are talking about. Out of that has come a reference payload capability. However, the interaction of these efforts with the science community has not crystalized in the same way that the interaction is now crystalizing with the MJU Science Advisory Committee. I think what has happened is we find ourselves looking at the reference payload as being the payload for this mission. Let us not do that. Let us wait until we get more specific inputs from the science community.

I also heard some very interesting stories about different mechanizations on the mass spectrometer, and it is, obviously, a very interacting instrument with the probe system design. Let's wait until we get the real inputs from the science community before we settle on the specific design of the Uranus probe. I think we need this interaction and I think that we'd be playing the wrong game not to let the scientific community give us their best inputs and their druthers, and then let's look at the probe design and see how best we can accommodate their desires. I think that is what Toms is pushing here.

MR. VOJVODICH: I would like Larry to speak to that issue.

DR. COLIN: I certainly endorse the idea of science groups continually reviewing the situation. We have been pushing for that sort of thing and it hasn't occurred yet. But I am hoping that Ichbiaque Rasool will get it rolling. As far as the model payload is concerned, it is in very fine shape. I personally doubt that there are going to be significant modifications to it.

MR. SPERANS: I think there is a misunderstanding here. I think that if anyone thinks that this payload was derived by a few people from Ames and a few contractors sitting in a back room and deciding what would fit into a probe, they are very much mistaken. We have had interaction with the science community right from the very start, dating back four or five years. We've had science advisors representing a cross section of outer planet scientists all along. And it has been their input which has dictated the sort of payload that we are talking about today. The implication that we have been working without this sort of thing is in error. There is only one difference between this and MJU and that is that as yet we don't have a formal Science Steering Group. And the reason for that is programmatic and I am sure that when the time comes, Headquarters will set one up.

MR. SEIFF: There is, for example, the benefit of the entire process by which the Pioneer-Venus payload was defined, which is the usual excruciating process by which people submit - I think there were 180 proposals submitted to fly experiments on Pioneer-Venus and it got narrowed down to what is now an instrument count of thirty-three but there are actually fewer investigators than that. So that what is being done here is all of this experience is being factored forward. Now you do have to admit the possibility that the selected payloads to the outer planets will differ. But neither should what is being shown here be regarded as something that was selected blindly without guidance.

MR. HYDE: I don't mean to imply that. I was specifically trying to get to this point: Let's not kid ourselves and say that this reference design that we currently have is The Design. We have to remain open at this time.

MR. SEIFF: Yes, I am quite sure that when it is executed, it has to be done that way, because nobody would sit still for any other approach.

MR. SPERANS: Well at the same time we keep talking about trying to do low-cost missions and sooner or later we are going to have to face up to the fact that if you are going to do anything remotely resembling a low-cost mission, you have got to settle on some kind of a fundamental science objective and set out to do it, and stop trying to optimize it right up to the point of launch. I think this is one thing we are going to have to live with from now on.

MR. SEIFF: Howard has been trying very eagerly to get in.

MR. MYERS: I would like to make a few comments about upper-atmosphere versus lower-atmosphere instruments.

I wish to comment on the desire expressed by the atmospheric scientists for upper atmosphere measurements. Under contract to ARC, we studied the accommodation of upper atmosphere instruments to Outer Planet probes. We found that the installation of a simple instrument such as electrostatic probe presented no difficulty. Its data could either be transmitted in real time or stored for postblackout transmission. A neutral or ion mass spectrometer can also be added. However, the problems of calibrating an upper atmosphere mass spectrometer

described in Dr. Nier's paper are aggravated for the Outer Planets by the high entry velocities. Therefore, in the Science Instruments Caucus, the three mass spectrometrists recommended that mass spectrometry be limited to the lower atmosphere. The most promising additional instrument would be a second rf transmitter; the use of two-frequency radio data in atmospheric characterization was discussed yesterday by Dr. Croft.

A second aspect of obtaining upper atmosphere data deserves attention, that of measurement time. The total time available for upper atmosphere measurements (that is, from onset of a sensible atmosphere at $10^{-7}G_E$ to $10^{-2}G_E$) is 20 seconds for a shallow Jupiter entry and up to 30 seconds for Saturn and Uranus! Therefore, the intrinsic value of 30 seconds of upper atmosphere data must be weighed against the increased complexity imposed upon the probe design.

MR. SEIFF: There is one point that was brought up by Phil Nachtsheim - that I would like to see aired a little bit because I think it is so sensible that it probably would be thrown out without consideration, and that is that since we have problems trying to define the capability of heatshields to survive Jupiter entry by any means here on Earth, one might conceivably undertake something very modest, small in size, carrying a minimum number of instruments and throw it off of some vehicle that happens to be flying by there, such as Mariner-Jupiter-Uranus. And not expect too damn much of it; just use it for a learning experience and if we are estimating forty-eight million dollars for this device, the question that comes into my head is what could be done with five? What could be done with five and how much of a leg up would it give us on this problem to take the risk out of the really more capable mission? Now I would like to hear other people's opinion about this. To me it seems exceedingly sensible.

MR. VIC PETERSON: Al, it is conceivable that with a sum of money much less than five million dollars we could accelerate the development of the Jupiter arc facility. This would enable us to simulate the entry environment here on the ground and be able to run the experiments over and over again rather than depend on a one-shot thing.

MR. SEIFF: That would be delightful if true, but I think Howard Stine's report to us was not one really bubbling over with optimism.

MR. PETERSON: He is trying to be realistic.

MR. SEIFF: He is trying to be realistic and what he is saying is if we can marginally obtain the conditions of interest and rather late in the game, and on a rather small sized specimen. But if your speculation were true, Vic, I think it would be the right way to go. Now I haven't seen evidence that it is correct. That's the thing that's bothering me right now. It looks to me like we can invest that same kind of money and still end up somewhat short of what we would like to have.

MR. PETERSON: It is true, though, Al, that you will always get something out of a facility. With a probe you have a fifty-fifty chance of getting nothing.

MR. SOMMER: If it fails you will get something; you will know that your design was inadequate.

MR. SEIFF: Does anyone else wish to comment on that?

MR. SWENSON: If you forget the launch vehicle, your five million dollars will be all right.

MR. SEIFF: Well, that is what I am saying, that this has to be a piggyback experiment on some other mission.

MR. NIEHOFF: I would like to give you a counterpoint to your five million, based on the forty-eight million that we talked about earlier. That was for three flight articles. And if you remove two of them, you are more like thirty-eight million. If you knock off all the science and all the communication, which is not reasonable - presumably, even with a test you want to get data back after you have entered to find out what has happened - you would knock off another seventeen million, so you are down to about twenty million.

Presumably, this thing would be smaller and there would be some savings associated with that; but I still would have to believe that five million is probably unacceptably small.

In fact, I would propose that we start off with five and the way this meeting is going, we will wind up at baseline payload by just normal procedure.

MR. SEIFF: Yes, but you know how everybody's ruminations, it doesn't mean we are going to have -

MR. NIEHOFF: Be careful, seventeen million dollars of that is in communications and science.

MR. SEIFF: But you can shrink your communication system, too, because if you take out the major part of the science -

MR. VOJVODICH: That is his point.

MR. SEIFF: Is that your point?

MR. NIEHOFF: Yes.

MR. CARL HINRICHS: One should be a bit cautious in scaling the costs of communications systems. Regardless of the data rate or range, the link analyses must be performed, i.e., look angle

and range histories, error assignments and modulation/coding investigations. Similarly the procurement cycle costs are somewhat invariant, i.e., assessment of EMC and vibration/shock/acceleration environments and the associated testing costs. Even with the use of an "off-the-shelf" system, these same steps (costs) must be traversed, although hopefully with some of the steps deleted. It would be interesting to see Mr. Niehoff's data broken into recurring and non-recurring costs on a per link basis.

MR. SEIFF: I'm quite serious in being interested in that idea. I don't know whether anyone else feels that way or not, but to me it seems like a very real suggestion. Any other comments or questions?

STAN LIPSON: Will you make a few remarks concerning what role you see ESRO playing in the Pioneer-Jupiter orbiter mission?

MR. SEIFF: Larry (Colin) can you answer that, or John (Foster)?

MR. FOSTER: That is not an entry mission and I'd just as soon defer that, unless Paul (Tarver) wants to answer. That's a Headquarters problem at the moment.

MR. TARVER: This is one of several possible cooperative missions under discussion with ESRO. Conceivably, one role ESRO might play would be to convert the Pioneer H spacecraft into an orbiter with science instruments supplied by both ESRO and NASA. Again, this is just in the early stages of talking about it. But we have a Pioneer H spacecraft, and if this were to be furnished to ESRO, it could be converted into an orbiter. As to how a

probe would be handled if there were a probe, this is totally unresolved.

MR. SEIFF: Was there another question? I think we have wound down. We have been going at it for three days and that point has been reached where nobody can think of anything else to say.

I would just like to say in closing that while I wasn't instrumental in putting this meeting together, I really feel gratified that it was held. I think that it had a number of very positive effects. Some people have been calling for closer interaction between scientists and design groups and we had that here.

I have attended meetings on both sides of that fence, but I have never been to a public meeting where there was really quite as much exchange as I have seen here.

Another thing that I thought was extremely healthy was the fact that we had contractors talking to each other. So we have had contractors and we have had Headquarters people and Center people and scientists all communicating with each other.

To me, the whole thing has been very much worthwhile. I don't feel sorry at all that I spent three days sitting here, and I hope the rest of you feel the same.

And with that, I will declare the meeting adjourned.

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SECTION D

ATTENDEES

OUTER PLANET PROBE TECHNOLOGY WORKSHOP
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